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# BOTANY.*

By Herbert Maule Richards.

As you have heard in previous lectures, there is an increasing tendency on the part of biologists to segregate less sharply the physiological and morphological fields of work, to take a broader view not only of the content but also of the methods of the two branches of biological investigation. It must not be supposed, however, that in this tendency towards coöperation there is a return to omniscience of the type of the old-time naturalist, who by reason of the lack of detail was able to consider himself proficient in many branches of science. The modern morphologist must still be a morphologist, and the physiologist a physiologist, only he has a broader point of view and does not hesitate to avail himself of the cognate branches of his science, or of any other science, where he feels that he can further the aims of his researches; he is an eclectic and picks that which will serve to advance his work along the most fruitful lines.

Almost any investigation of wide scope is in these days an example of this improved attitude, but no other perhaps illustrates so conclusively what may be called the highest type of modern research as does the development of the Mutation Theory first propounded by de Vries. What de Vries has really done is to bring within the range of experimental proof certain questions which heretofore have been regarded as matters of observation and speculation alone. From this point, which might be said to have had its origin in the acuteness of observation of the taxonomist and morphologist, the physiological trend has

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ever increased until the last word in this discussion may perhaps be for the physiologist alone. The great question involved in the Mutation Theory is the old, old problem of the origin of species, a very considerable advance in which has been made by de Vries and those who were stimulated by his work. It is quite wrong to suppose that he has controverted the general results of Darwin's work; he has supplemented it, brought it within the range of more conclusive proof.

As the Linnaean or collective species may be regarded to-day they are usually separable into several more or less distinct strains which show no intergrading forms, and the diagnosis of any one species is, so to say, the average impression of them. To these distinct strains de Vries has given the name elementary species, and according to his interpretation they are the really discrete, finally segregable units, between which no intermediate types exist and concerning the origin of which we are really concerned. It matters not whether it was through ignorance or simply from convenience that the earlier taxonomists grouped many of these forms into a single species; we must conclude, that in general species, as recognized by the books, are quite artificial. It matters not, also, what we call these finally not further resolvable forms. Therefore let us accept de Vries's terminology and use the term elementary species; the real point of the inquiry is how did these forms arise. It is upon this that de Vries's work has thrown a great light. He has shown that they may arise suddenly and without previous preparation from preëxisting forms, in which case the elementary species may be termed mutants, and the theory which has to do with the investigation of their origin the Mutation Theory.

The next task then is to examine more closely the methods which de Vries employed, the evidence which he has to support his views, both as to the observations on the origin of these mutants and their behavior after they have come into being, and further, what success subsequent investigators have had in supporting de Vries's evidence, and how far they have extended his conclusions. In the first place, it may be remarked that the conclusions as first published in 1901 and 1902 were not the

outcome of any hasty experiments and ill-digested data, but were the result of seventeen years of the most careful and painstaking work, and a fine example of the best kind of quiet, faithful research, removed from the rush of affairs and the demand for immediate results, the final conclusion of which fully warranted the time and labor expended.

As is well known, Professor de Vries found in Lamarck's evening primrose—*Oenothera Lamarckiana*—a plant most favorable for observation, though his conclusions are not based on that form alone. The most carefully guarded pedigree cultures were made from the true *Lamarckiana* type, and the astonishing result developed that among the offspring of these certain forms, to the number of about 4 per cent., showed new and striking differences. In all, more than a dozen new forms were obtained which, if they could be bred at all, bred true to their new characters and did not revert to the ancestral *Lamarckiana;* these were the mutants, the new elementary species, which had sprung suddenly in a saltatory fashion from the parent stock. The great importance lies in the fact that they were entirely constant to their new characters and were thus not in the class of the merely unstable varieties. It must be remarked that time alone, many generations, of carefully guarded cultures in which accidental crossing was an impossibility, together with unimpeachable records, could adequately establish this momentous fact, that here was a new species, a new form, or whatever you may elect to call it, which had sprung all in one jump from its parental stock. De Vries, then, was the first man who ever saw a new type of organism come into the world and who recorded its advent.

You naturally ask how unlike were these new forms, a question which is difficult to answer without actual illustrations. However, it may be said that many of them were different enough from their parent stock to be admitted by taxonomists to come within the definition of new species, as species are regarded at the present time. The differences are not the question of mere stature, but of the whole habit of the plant and of the details of the form of both leaves and flowers. But to repeat, it really makes no odds

whether the differences are of such quality that they must needs be recognized as specific by taxonomists; what is important is that they are differences which do not intergrade one with another and which are inheritable in the second, third, and subsequent generations, and that no tendency to revert to the parent form is to be observed.

The results of de Vries have been verified by cultures in this country of his own and of other stock, so that there can be no question that this Lamarck's evening primrose behaves in its manner of mutation the same here as elsewhere. More than that, other mutating forms have been discovered, and by the application of biometric methods much that is important regarding the relative variability of mutants and their parent stock has been determined. Besides the actual experimental work, the history of Lamarck's evening primrose has been traced back for more than a century and a mass of inferential data is being accumulated which helps to support the main conclusions. Important as all these advances are, the most brilliant result is that obtained along the lines of the induction of mutations. By injecting into the developing ovary of a plant allied to Lamarck's evening primrose reagents which might produce a chemical or osmotic effect upon the cell contents, MacDougal has actually succeeded in inducing mutations. The seed grown from the stimulated plant may produce forms quite distinct from the parent type and, what is essential, the mutations thus induced are constant to the second and third generations. That such a result can be obtained is simply astounding when one considers how firmly an organism is bound by its heredity. It would appear that a tremendous shock had been given the plant at a critical period in its life history which has enabled or forced it to break down some of the minor barriers imposed by its hereditary tendencies and to erect new ones, which circumscribe its offspring as the original ones did its parent. As to the precise nature of this shock we can at present only speculate, but it is permissible to suggest that it is perhaps of the nature of the rearrangement, in a chemical sense, of the protoplasm of the cells of the sexual generation. As to the natural production of mutants, given such a conception of the nature of the process

involved, it is possible to suggest various ways in which it might have been brought about.

The line of departure of mutants from the parent type is not in any one direction, and the manner of variation appears to be wholly a matter of what we are pleased to call chance. As has been said, de Vries obtained more than a dozen different forms. Some of the mutants, we may say, are probably destined to failure, others perhaps are better placed, at least in new environment, than the parental type and might conceivably stamp it out in time. What the criteria of success or non-success may be is a matter upon which no one would care to give an opinion, but I have in mind the fact that one of the mutants of Lamarck's evening primrose has a tendency to germinate somewhat more quickly than the parent form, and the seedling grows a little more rapidly; it is conceivable that some slight advantage of this sort might be the crucial point. However that may be, it is here that we can apply the Darwinian concept of the struggle for existence, a struggle however not between single individuals, as the idea of continuous variation would imply, but the struggle between great numbers of individuals, whole groups of elementary species. The great contrast between Darwin and de Vries is the contrast between the slow and continuous accretion of variations implied by the former and the sudden jumping or saltatory variation insisted on by the latter. By such means as de Vries maintains the process of evolution might take place with far greater rapidity than by Darwin's method, for, generous as the geologists are in their allowance of time for the development of organic life on the world, it has always been difficult of conception how even the countless ages granted could compass the enormous development of the highest organic types from simple forms. To maintain that de Vries's theory is entirely complete, and must be the only means of the origin of new forms, is unnecessary. None but the extremists would go to such a length; it is not at all necessary to assume that the means to a similiar end must necessarily be similar. What may be maintained, and properly so, is that mutation constitutes one way, at least, by which new forms of organisms may arise on the world's surface. New forms, in the

sense of the new combinations of old characters which come into being by reason of stable, non-reverting hybrids, are known to have originated, but such new forms imply of course the preëxistence of varied types, and do not have to do with the question of the origin of new characters.

It is not in the order of things that a new theory of such import as the Mutation Theory should not find opponents. These I think may, in the main, be grouped in three classes. First, the critics who doubt the evidence, who can be answered by referring them to the printed records, and recommending a repetition, as careful as the original work, of the experiments which have led to the new point of view. Second, those who quibble concerning terms, and this type I think constitutes the majority, who will likely suffer the fate that is usually meted out to quibblers, that of being ignored. Lastly, those opponents who, while they may not doubt the accuracy of the work doubt the conclusions on philosophical grounds. These are the critics whom the advocate of the de Vries Theory must welcome and who will arrest his sober attention, for they will stimulate him to accumulate more and more evidence to support his position. Even were I able to analyze adequately the controversial side of the question for you, it is obvious that time scarcely allows, and I will, in consequence, state frankly that the account which I have presented is from the standpoint of an advocate of what the Mutation Theory teaches, and add that I am not aware that any experimental work has controverted it. Let me say, however, and here I wish to speak for myself alone, that I cannot see it makes great odds whether fifty years hence or five years hence we accept the Mutation Theory just as propounded by de Vries. The great point is that an advance has been made, the most important advance since the time of Darwin, by way of helping to elucidate one of the great questions in which man is interested. It is not to be supposed that we have as yet any final answer to this question; final answers are not indeed the goal of any one scientific research. It was Sir Isaac Newton, I think, who said that the seeker after ultimate causes did not show the true scientific spirit, and he was right. What we have is one of the proxi-

mate causes demonstrated to a degree which had not been previously attained. A scientific theory is like an organism, it grows and it may also propagate itself, and all the theories of evolution from Lamarck to de Vries, and those that will follow, will themselves be an example, as it were, of the principle that they teach. A theory which starts life an intellectual pigmy, may develop, if it have the vitality, into a veritable intellectual colossus, and, after it has run its course, may leave behind its offspring. It is not a cause of reproach but rather of congratulation that the scientific theory of to-day may be discarded to-morrow, for no theory will be abandoned until a better one has been brought forward to take its place, one which can explain the facts in a way more satisfying to the human mind. Change in such a case is progress, and since science must of necessity be always progressing so also must it be always changing.

To those who are conversant with the problems connected with the origin of species, it must be obvious that this consideration of the subject does not cover the whole ground; so obvious, indeed, that perhaps it is unnecessary for me to remark that it is not intended to. There are other theories to be considered and other equally important matters that are more or less interwoven with any one theory of the evolution of new forms. Thus no reference has been made to Mendel's researches on heredity, or the way in which they touch upon the de Vries Theory. This has been omitted purposely, for while the results of Mendel's original experiments in the breeding of peas might be cited at length, I doubt if an apter or more significant example could be found than the one which Professor Wilson used, and as Professor Wilson himself said, the explanation while not abstruse is one that requires considerable preparatory consideration. The Mutation Theory has been developed more in detail, as representing a type of research. Being one of the latest and most important contributions to biological science, and being also entirely germane to the subject in hand, it has seemed proper to devote some time to its consideration. At many points do the fields of modern botany and modern zoölogy touch, but perhaps it is nowhere so evident as in great problems like these. Here

the two sciences work in generous rivalry, each eager to add its contribution to the store of general knowledge, to utilize such information as the sister science brings, to criticize it if need be, but always to accord it a respectful hearing.

(*To be concluded.*)

## STUDIES IN THE OPHIOGLOSSACEAE—II

### A Descriptive Key to Botrychium in North America: Group of B. lanceolatum

By Ralph Curtiss Benedict

The present treatment is designed to set forth briefly the essential facts of our knowledge of these plants from a taxonomic standpoint. The status of the various units included is not considered. The question of specific limits is a perplexing one throughout the genus, and one which will probably require cultural work, such as the raising of the various forms from spores, and under varying ecological conditions, to settle it satisfactorily. But additional information gained from field and herbarium study will be of value, and any corrections and additions to the account given will be welcome.

For convenience, the genus may be divided into two groups, typified in general by the species *B. lanceolatum* and *B. ternatum*, respectively, and characterized as follows:

Group of *B. lanceolatum:* Bud hairless (*B. virginianum* excepted); commonstalk one-half or more epigean (*B. pumicola* excepted), usually one-half or more the height of the plant (*B. simplex* excepted); spores maturing from late spring to early summer (May to June). Included in this group are the following: *B. simplex* Hitchcock, *B. pumicola* Coville, *B. boreale* Milde, *B. onondagense* Underwood, *B. Lunaria* (L.) Sw., *B. tenebrosum* A. A. Eaton, *B. neglectum* Wood, *B. lanceolatum* (Gmel.) Ångstr., *B. virginianum* (L.) Sw., *B. dichronum* Underwood.

Group of *B. ternatum:* Bud hairy; commonstalk hypogean, short, usually less than one-quarter the height of the plant; spores maturing from the middle of summer to early fall (July to October) (three exceptions).